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THE AMERICAN

METEOROLOGICAL JOURNAL.

VOL. IX. BOSTON, MASS., FEBRUARY, 1893. No. 10.

HOT WINDS IN TEXAS, MAY 29 AND 30, 1892.*

I. M. CLINE, LOCAL FORECAST OFFICIAL, U. S. WEATHER BUREAU.

GENERAL hot winds accompanied by abnormally heated currents appeared over the northern and western portions of Texas on May 29 and 30, 1892. Those of the 29th were so extensive and severe that, while hot winds occur to a greater or less extent every year in parts of the State, the oldest inhabitants have no recollection of anything like these in any previous year. Attention was first attracted to this subject by short notices in the daily papers speaking of the damage resulting from the effects of these winds, and then by reports from special correspondents of the Texas weather service. Their unusual extent and the damage resulting therefrom showed that they deserved more than ordinary attention, and circulars were sent to a large number of post-offices in the affected district requesting all the information available, relating to and which would enable a study of these winds.

The area from which the hot winds and currents were reported covers more than two thirds of the State, and includes about one hundred and seventy-five counties (about one-seventeenth of the United States, Alaska excluded). Their eastern limit coincided closely with the 400 feet contour line, and their southeastern and southern limits with the 500 feet contour line, of elevation. While the day was abnormally warm over the affected district, the intense heat was not general but traveled in narrow currents, ranging from 100 feet to half a mile or more in width, and several miles sometimes intervened between the

* By permission of the Chief of the Weather Bureau.

currents ; in fact several counties did not experience the heat to any extent worthy of notice.

The hot winds made their appearance on the 29th about 2 P. M. in western portions, and about 6 P. M. in eastern portions of the State, and continued in some sections during the 30th. The damaging effects resulted from the hot currents which appear to have been most extensive about or after sunset. Some correspondents state that the hot currents were distinctly visible. Mr. Silas G. Lackey, a very careful and reliable observer at Mesquite, Dallas County, says they looked like sand storms going across the prairie, and states : " These hot currents would almost stop one's breathing. I was caught in the centre of one about 100 feet in width and it was almost insufferable." In speaking of one of the larger sized currents Mr. Lackey says that it could be seen for miles after it had passed north of his place, and that its top was visible, from where he was standing, to an altitude of about thirty degrees, and its base was about four miles distant. Similar currents can be seen nearly every summer on the prairies of western Texas. A tremulous motion can be seen in them to a height of several feet, something like that seen in the atmosphere above a heated furnace.

All vegetation in the paths of these currents suffered severely, but the greatest damage was to cotton, wheat, and corn crops. It is estimated that, approximately, at least 10,000 acres of cotton were destroyed and a great deal more damaged. The cotton plant had the appearance of having been wilted to the roots by intense heat. The wheat was in dough, and was cooked and dried up to such an extent that the average yield was much less than was expected from the general appearance of the crop during May. Corn suffered severely in some localities, but it was so hardy and well rooted in most sections that it recovered to a great extent from the injurious effects. Timbered sections did not suffer any of the bad effects, in fact none of the heated currents were observed in the vicinity of timber, as was noted by some of the observers who report hot currents.

In connection with these winds we will examine briefly the meteorological conditions under which they made their appearance, and note the more important features presented by the weather charts for two or three days previous to their occurrence. On the 27th a cyclonic disturbance was central over

Assiniboia with pressure below 29.30 inches, and a trough of low pressure extended southward over the eastern Rocky Mountain slope to western Texas, but an anti-cyclone was central in the Pacific Ocean off the coast of Oregon. Generally, cloudy weather was prevailing over the Rocky Mountain region and westward, with rain over eastern Montana, the southeastern Rocky Mountain slope, and western and central Texas. The high pressure was apparently forcing the moist air from the Pacific coast across the divide toward the region of low pressure. On the 28th the storm was central over Manitoba with pressure still below 29.30 inches; the trough running southward had deepened, and the pressure at Abilene, Texas, was about 29.70 inches. The high pressure continued off the coast of Oregon, and the tendency of the atmosphere to cross the divide was more marked than on the previous date, and the general movement was toward the southeast. Cloudy weather continued over the Rocky Mountain region, and for some distance east and west except over the southeastern slope where it was generally cloudless. Rain was reported from the northern and central plateau regions and the central valleys, and thunder showers over central Texas. On the morning of the 29th the cyclone had decreased in intensity, and spread out over the country from Manitoba to the lakes; a loop with conflicting winds showed a tendency toward the development of a secondary cyclone over Oklahoma and Kansas. Rain had fallen over the plateau region south to Arizona. On the afternoon of the 29th, the primary cyclone remained about the same as at the morning observation, but a well defined secondary had developed over Oklahoma and northwestern Texas. On the 30th the primary cyclone moved eastward to the north of the lakes and disappeared, but the secondary remained almost stationary in the position occupied on the evening of the 29th until the morning of the 31st when it moved off to the northeast.

The most intense heat was experienced on the afternoon and during the night of the 29th, but the hot winds continued during the 30th with less intensity. At the time of the appearance of the hot blasts, observers in nearly all sections from which damage is reported state that clouds formed and disappeared rapidly, and several report thunder. Some state that at the time these conditions made their appearance a general and sudden hot wave

was experienced but with much less intense heat than was observed in the hot currents. The temperatures reported on the 29th and 30th were unusually high, and the maximum thermometer ranged generally from 90 to 100 degrees, and along the western border from 105 to 109 degrees. The hot wave is said to have been so intense in localities over the western portion of the State that in a few places on the Southern Pacific railway the rails expanded under the abnormal heat to such an extent that they "sprung," and trains could not pass until they were shortened and replaced. In connection with the appearance of the hot wave, on the 29th, Mr. C. F. Mercer, Arlington, Tarrant County, who has proven himself a careful and reliable observer since the organization of the Texas weather service, has furnished observations of the exposed thermometer for each ten minutes for about one hour. At 5.20 P. M. the thermometer registered (Fah.) 89 degrees, with wind south-southwest, and at 5.30 P. M. read 96, showing a rise of seven degrees in ten minutes. After this, general cloudiness set in and the temperature fell steadily until 6.25 P. M. when light rain began falling. Mr. Mercer states that the nearest to his place of any hot blasts, which killed vegetation, was about three miles east.

Hot winds occur to a greater or less extent in Texas every year. They rarely occur, however, with sufficient intensity to injure vegetation earlier than the latter part of May, or later than the middle of September. Those who have observed them state that their characteristics are generally the same. Mr. C. Tompkins, a special observer of the Texas weather service, Anson, Jones County, in speaking of these winds says: "They are of annual occurrence in this section during June, also sometimes earlier and sometimes later. This is the crucial test on our corn, and on this account it is considered the most unreliable crop here. The hot winds usually come from a westerly direction and are very hot and dry; crops wither; the flesh becomes dry and very hot with no moisture on the skin. In eleven years' residence here we have had but one or two seasons without these winds. We expect them yearly." The appearance of these winds has generally been noticed during the day; but in the present case, which is the most extensive on record, they were more general in the earlier part of the night than at any time during the day. Many sections report considerable damage where hot currents

were not observed; but from the description of the vegetation injured, which corresponds with the effects described where the currents were visible, the cause must have been the same and the failure to observe the hot currents can be accounted for by their appearance during the night.

The origin of these winds, or the causes which produce them, is of the greatest interest, as without this knowledge it would be difficult to forecast them or take any steps with assurance of success to protect crops from their effects. The hot winds of Texas must emanate from the same source as those which occur in Kansas and other States to the east of the Rocky Mountains, regarding which different theories have been advanced during the past few years. The theory supported by Mr. G. E. Curtis, in his study of the hot winds of Kansas, which attributes their origin to local heating of the earth's surface, will not always hold good with the hot winds of Texas; and it is a question, which future investigation will have to decide, whether it will apply in any case to the true type of hot winds in this State. The winds in the present case are remarkably free from preceding or accompanying conditions which would justify any claim attributing their origin to the local heating of the earth's surface. The precipitation for the month of May was above the average in all parts of the State where damage was done by these winds, the amount was well distributed during the month, and showery weather was reported generally for two or three days previous to their occurrence. In addition to a wet soil the appearance of currents of air with higher temperatures than that of the general atmosphere (which was, however, suddenly heated at the time of their appearance, but not to a heat equal to that manifested in the currents themselves), is sufficient to do away with any claim to an origin from local heated areas. These winds appear to have resulted from the same causes which produce the *foehn* winds of Switzerland, described by Dr. Hann. Such high temperatures as evidently existed in the hot currents referred to must have been derived from dynamic heating; and the sudden rise in temperature reported by some observers must have been from the same source, as under existing conditions this could not have been caused by local heated areas.

We have observed that the general movement of the atmosphere for three days previous to the occurrence of these winds

was from the Pacific Ocean across the Rocky Mountains, mostly in a southeast direction, and that during its ascent from the coast to the divide, the condensation of moisture, either in the form of cloud or rain, was steadily in progress. The latent heat thus liberated by the process of condensation reduced the loss of temperature sustained by the atmosphere in ascending the mountains to nearly one half what it would have been in the case of dry air. This left the air to descend dry on the eastern side of the mountains, and after it had descended far enough to disperse the cloud carried over, the gain in heat during the remainder of its descent would be nearly twice the loss sustained during its ascent. At an elevation of 6,000 to 12,000 feet above sea-level the temperature of the atmosphere was only ten to fifteen degrees below that at an elevation of 400 feet. The vertical decrease in temperature was apparently not very great, and in response to the needs of the cyclone to the eastward of the Rocky Mountain plateau, the dry air from this elevated region would move out toward that area in a direction governed by the circulation of the winds around the low-pressure area. The circulation shown on the weather maps would tend to carry the air from the central and southern plateau regions out over Texas, and the development of the secondary cyclone to the north of Texas on the 29th, with brisk to high northwesterly and westerly winds in its western portion, would tend further to carry this dry air down over the portions of Texas affected by the hot winds. The general hot winds which, judging from our reports, appear to have reached the western portions of the State about 2 P. M., and the eastern portions about 6 P. M., can be attributed to the general dynamic heating of the atmosphere during its moderately rapid descent down the eastern slope; while the occurrence of the hot currents can only be accounted for as caused by dry masses of air in the upper strata, with a slightly greater density, being drawn out over the moister and less dense air near the earth's surface, thus causing rapidly ascending and descending currents as indicated by the rapid formation and disappearance of clouds. Currents of air thus descending rapidly would become abnormally heated dynamically to a much greater extent than where the descent had taken place slowly, as in the case of the general descent of the dry atmosphere down the mountain side. The descending currents

must be large, or their heat would be lost in intermixture with the ascending currents. And they must be rapid, or their heat would be lost by radiation. The cessation of the heated currents in some localities with the appearance of nearly general cloudiness, while they continued in other localities where clouds were few, may be attributed to a more general intermixture of the ascending and descending currents which tempered the descending currents ; the general cloud formation would indicate such intermixture.

The importance of protection from these winds may readily be inferred by the remarks quoted from Mr. Tompkins in reference to the corn crop as well as the effects of the winds in the present case on the wheat and cotton crops where they were experienced. The planting of timber has been suggested by all who have given any attention to this subject. It is noticed in the remarks of correspondents that no damage resulted in the vicinity of timber, and it is believed that if every farmer would enclose his farm with a hedge of timber that he would be protected to a great extent, both from hot winds and northers. The tree used should be one productive of fruit, and in this respect it would be a very valuable addition to his farm. The pecan and olive might be used for this purpose to great advantage in many parts of Texas.

U. S. WEATHER BUREAU,
GALVESTON, TEXAS, Oct. 1, 1892.

THE ELECTRIFICATION OF THE LOWER AIR DURING AURORAL DISPLAYS.*

ALEXANDER MCADIE, M. A.

IN a paper published elsewhere,† an account is given of some experiments made in July and August, 1892, at Blue Hill Observatory, where, by means of a kite flown during thunderstorms, we sought to obtain, among other ends, a better record of the potential of the air than could be given by a collector near the ground. To illustrate, we will take one date, Aug. 9. The kite, as described, was up in the air about 11 A. M., and

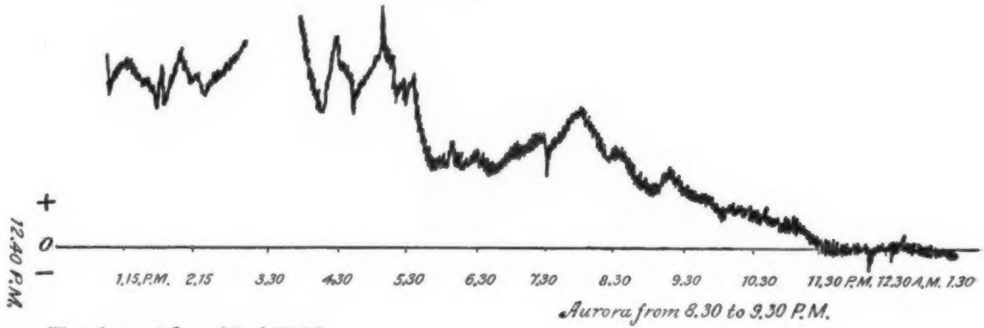
* By permission of Chief of Weather Bureau.
† Electrical World, Sept. 24, 1892.

remained up until after 10 P. M. At about 7.40 the thunderstorm, which had been noticeable in the west for some time, was near enough to cause sparks without cessation from the end of the kite wire. A constant sizzling was heard within the large multiple quadrant electrometer, to which the end of the kite wire was connected. On lifting one of the metallic screens in the side of the instrument, the quadrants were seen to be aglow. The light reflected from the tin-foiled sides of the case was strong enough to cast shadows. Simultaneous with one flash of lightning in the west, quite a strong shock was received while happening to touch the wire. The thunder was heard in less tban five seconds after, so that if we call the average temperature of the air between the cloud and earth sixty-five degrees, the flash would not be over seventeen hundred metres away at the nearest point. Taking the kite wire from the electrometer, we placed a 105-volt lamp on the kite-string, and brought a ground wire near it. The lamp flared, and seemed full of diffused light. It was not a case of the filament becoming incandescent; but, rather, it seemed a case comparable with that instanced by Maxwell as made by Cavendish.*

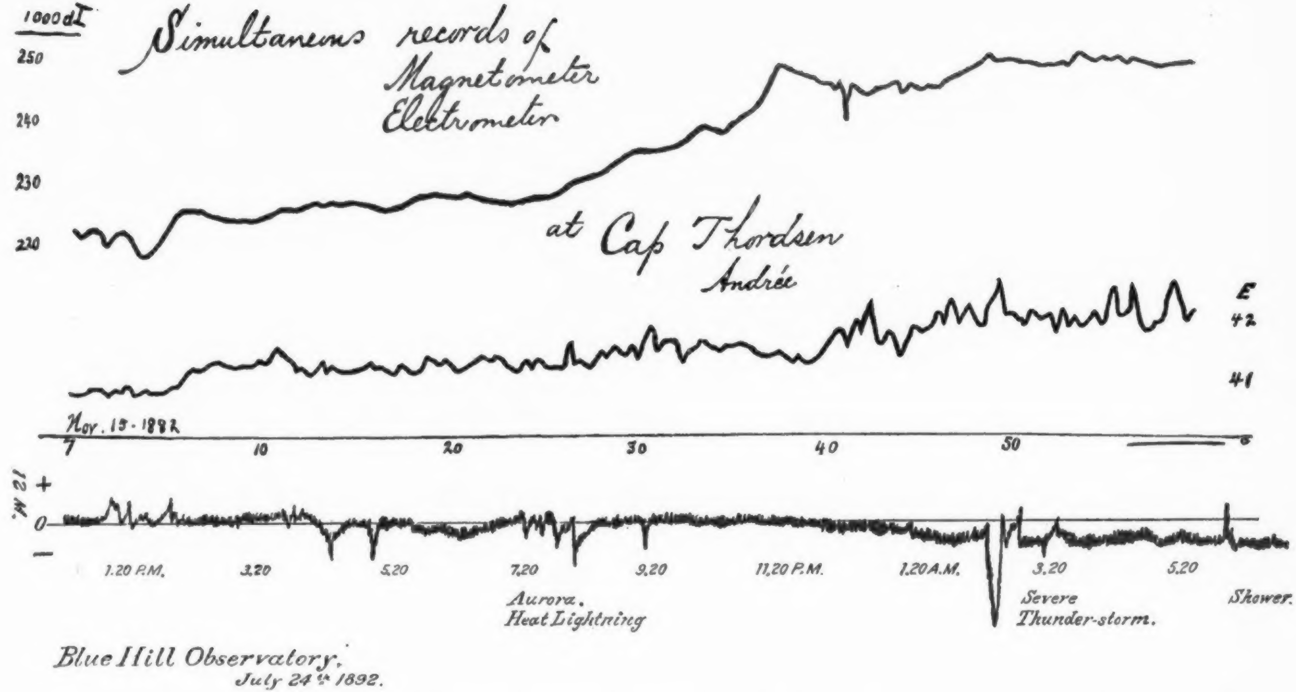
At first, it seemed as if these flarings were synchronous with the lightning, but after a while we saw the lamp flaring when there were no apparent discharges. Of course there may have been flashes unseen by us. Stinging shocks were felt on handling the wire at this time. Holding the lamp in the hand, and touching the wire with the glass end, the diffused light again appeared. Rain began about 8.10, and was over in a few moments. To the northeast, over Boston, Lynn, Marblehead, etc., there was a great display of lightning, with occasional strong discharges. The newspapers of the next day show that considerable damage was done by the lightning in the vicinity of Boston. Yet we could not hear thunder, nor photograph the lightning. And what seemed more noteworthy still was that, although the electric phenomena were to be thus plainly seen at a distance of from ten to fifteen miles, there was no sparking at the end of the kite-string wire, nor any flaring of the lamp.

* "In measurements of the capacity of glass plates, tin-foiled, Cavendish noticed that the charge seemed to spread beyond the limits of the tin-foil. He charged his plates in the dark (method not given), and faint light could be seen all around the edges, etc." — *Electrical Researches of the Hon. Henry Cavendish.*

Atmospheric Electricity.



Friday, May 23rd 1890.



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9.20
11.20 P.M.
1.20 A.M.
3.20
5.20
Aurora.
Heat Lightning
Severe
Thunder-storm.
Shower.
Blue Hill Observatory,
July 24th 1892.

This impressed us the more, as we are of the opinion that the storm, when at an equal distance to the west and south, had given marked effects. For reasons which need not be gone into here, we do not think that the slight rainfall was the determining factor.

This observation seems to bear so directly upon the question of the electrification of the lower air during an aurora, that I have collected whatever observations I could come across bearing upon the matter. I also give some electrometer records showing the potential variations during auroras, but whether due to the aurora or to some local cause, we confess ourselves unable to say.

At this particular time, auroral displays will probably be unusually numerous, and we hope the period will not pass without some increase in our knowledge concerning them. No reader of the JOURNAL need think that, because he has no electrometer and no laboratory, he can do nothing to help in the matter. If one will fly a kite to a considerable height some night during an auroral display, and have a fine copper wire wound around the kite-string, and the ground end carefully insulated and put in connection with a metallic vessel from which a fine stream of water is flowing (the vessel being well insulated), he may possibly see the stream twist, and become distorted, under the influence of the electrification.

John Canton, in 1753, wrote, "The air without doors I have sometimes known to be electrical, but never at night except when there has appeared an aurora borealis; and then but to a small degree, which I have had several opportunities of observing this year."

Ronayne in 1772: "I have often examined the state of the air at the time of an aurora borealis and could not discover any indication of electricity except when a fog had appeared at the same time, in which case the electricity has been in every respect the same as that of a fog at any other time. Once indeed during an aurora borealis on a remarkably serene night, I discovered some signs of a very weak positive electricity." Carvallo on Sept. 14, 1775, had a kite up from 3.30 P. M., and found the electricity positive and pretty strong, the index of the electrometer being deflected about twenty degrees. The weather was cold and thick clouds were approaching. The kite was

pulled down at 4.30 P. M., and at night there was a brilliant aurora. He gives it, however, as his opinion that the aurora does not seem to affect the electricity.

With a pith-ball electrometer Sir John Franklin* failed to get indications of electricity during an aurora; but with another form of electrometer he obtained some deflections, not very strong or regular, when auroras occurred.

Parry at Fort Bowen with a gold leaf electroscope connected with a chain attached by glass rods to the skysail masthead, one hundred and fifteen feet above sea-level, found no effect.

Dr. Allnatt, at Frant, during the aurora of Feb. 4, 1872, found the gold leaves diverge; and McClintock observed, on six occasions of aurora in Baffin's Bay, a strong positive electrification; and on three occasions at Port Kennedy.

In the Record of the U. S. Expedition under Wilkes, it is stated that in latitude 65 S., 125 E. longitude, on Feb. 9, 1840, an aurora was observed covering the northern horizon; and the gold leaf electrometer was tried, but as the instrument was not a sensitive one, no effect was observed.

Dr. Everett at Windsor, on July 1, 1863, made observations during an auroral display as follows: —

Time.	Mean.	Electricity. Highest.	Electricity. Lowest.	Clouds.	Wind.	Press.	Temp.	Depression of Dew point.
7.31 A. M.	+2.3	+2.3	+2.2	0	Calm	30.12	62.5°F.	2.5°
8.20 A. M.	+1.8	+1.9	+1.7	0	or	30.12	65.9	4.0
9.49 A. M.	+1.8	+1.9	+1.6	0	very	30.11	70.4	7.1
3.47 P. M.	+0.8	+1.0	+0.5	0	light.	30.08	81.3	12.1
8.58 P. M.	+1.6	+1.7	+1.4	0		30.12	66.6	5.3
10.29 P. M.	+1.6	+1.6	+1.5	0		30.12	60.6	3.3

The Astronomer Royal, Mr. G. M. Whipple, at Kew, after discussing the magnetometer disturbances during the great storm of Nov. 11–19, 1882, writes of the electrometer that "there was no particular disturbance of atmospheric electricity; . . . the tension was much higher and more variable during the dense fog of the succeeding morning."

The various circumpolar stations of 1882–3 give but little that is definite. With most of the expeditions observations of atmospheric electricity were among those which were optional. General Greely records in the "Lady Franklin Bay Expedition"

* This and the two following instances, given by Capron, "Aurorae and their spectra," page 63.

an unsuccessful attempt to use an electroscope during a brilliant display. Of all the Arctic parties, the Swedish one seems to have gone most thoroughly into the matter of atmospheric electricity. Andrée at Cap Thordsen used a Mascart quadrant electrometer. The following are probably the best contributions yet made in the way of observations: —

	P. M.	P. M.	P. M.	P. M.	P. M.	P. M.	P. M.	A. M.
Nov. 11, 1882.	8.50	9.09	9.53	10.14	10.55	11.10	11.55	12.55
	+0.37	+0.64	+0.81	+0.84	+0.64	+0.46	+1.74	+0.71
Nov. 15, 1882.	7.50	8.04	9.06	9.54	10.09	11.09		12.05
	+1.24	+3.74	+1.93	+0.79	+1.31	+2.49		+6.29
Nov. 26, 1882.	1.55	2.10	2.55	3.15	3.54	4.15	4.54	
	+8.44	+2.56	+2.42	+1.72	+1.44	+1.32	+1.70	

Nov. 27, 1882, — P. M.

1.05	1.54	2.15	2.50	3.16	4.12	4.55	5.54	6.08	6.55	7.10	7.47	8.10
+2.02	+1.06	+1.76	+2.96	+2.48	+3.26	+4.56	+0.08	+0.96	+10.18	+2.48	+0.44	+0.26

	P. M.	P. M.	P. M.	P. M.	P. M.	P. M.	P. M.	P. M.
Feb. 3, 1883.	6.56	7.07	7.54	8.51	9.07	9.50	10.12	10.51
	+6.7	+4.8	+1.7	+1.3	+1.3	+1.8	+1.3	+6.3
Feb. 7, 1883.	6.09	6.56	7.09	7.55	8.09	8.54	9.13	9.54
	+37.6	+1.6	+1.6	+1.3	+4.5	+2.7	+52.6	114.4
March 6, 1883.	7.51	8.06	8.30	8.40	8.52	9.10	9.53	10.09
	+21.4	+50.7	+15.1	+15.1	+58.4	+16.5	+21.9	+43.7

These observations fail to show, says Andrée, that the electricity of the air acquires a very high potential during auroras (as Dellman thought from his observation of Oct. 1, 1859). On the contrary the potential seems to diminish. The fall is often very large and rapid (see March 6 and Feb. 7), and the character of the change is fairly comparable with the disturbed curves during rain and stormy weather. It is also of importance to recall that of all the hourly observations, only 1.7 per cent were negative, and of all these (220), only *one* occurred when the sky was clear; and this was on Dec. 1, 1882, and was followed in a few moments by an aurora. Two hundred of these two hundred and twenty negative readings were in connection with precipitation, and the other nineteen with cloudy skies. Can it be, then, that the aurora is intimately connected with condensation of the water vapor at certain heights above the earth and that whether visible as cloud or invisible, nevertheless is a residence for a high electrification? We have shown experimentally how the potential increases with elevation. We would have, then, electrified masses of air in upper levels (we know that the lower

clouds are electrified, by their influence upon our electrometer records); and if these happen to cut the lines of magnetic force of the earth, especially at times of disturbance of the magnetic field, we can see how brilliant magnetic-electric effects would be produced.

Whether this be so or not, we need badly some new classification of the various phenomena now classed together under the name aurora. The classification of Weyprecht, viz.: *bogen* (arch), *band* (ribbon), *faden* (beam or thread), *krone* (corona), and *dunst* (haze or diffuse light), with the two types added by the International Polar Commission, while most excellent as regards descriptiveness, should now give way, at least, in part, to a classification based upon the *origin* of the energy. Possibly the time is ripe now for the suggestion that auroras are of two general classes: —

A. The intense, highly colored, and magnificently brilliant displays of the *coronal-curtain-streamer* type, seemingly of extra-terrestrial origin. These are magnetic or electromagnetic in character, and so intimately connected with solar phenomena that the appearance of the spots, the perturbations of the needle, and the splendor of the aurora might be used as checks in timing the phenomena.

B. Displays of far less intensity generally more or less diffused and we have reason to believe purely within our *lower* atmospheres in short of the nature of "*wetter leuchten*," weather lights.

Such a classification at least explains somewhat the differences in the behavior of magnetometers and electrometers during auroras. Many instances can be found in the records of the International Polar Stations of 1882, when auroral displays of considerable intensity were not accompanied by magnetic disturbances (or by feeble ones), and conversely auroras that exerted but small influence upon electrometer while great perturbations were recorded by the magnetometer.

U. S. Weather Bureau, Nov. 1892.

THE PAPER MILLS AND THE FISHERIES; PRACTICAL KONIOLOGY.

PROF. CLEVELAND ABBE.

THE manufacture of paper in this country is a matter that has always had a special interest for me because of my familiarity with the workshops of the firm of Smith, Winchester & Co., of South Windham, Conn., where, for over sixty years, my uncle has manufactured machinery for the principal mills of this country. It was, therefore, not a surprise to me when, in 1873, I received a letter from one of our largest paper manufacturers, stating that times would occur during which all the paper manufactured by the firm was liable to turn sour and spoil on their hands. The remedy for this I was able at once to suggest: the souring is due to the action of fungous ferments, the germs of which are floating in the air and resting continually upon the sizing and upon the unsized surfaces of the fresh, damp paper. Two remedies for this are practicable: one consists in killing the germ, which is easily accomplished by mixing a little of any cheap, essential oil into the pulp or the sizing, just as is done by the photographer in order to preserve his paste. The other method, which is doubtless cheaper in the end, consists in accelerating the drying of the damp paper. It requires several days at ordinary temperatures for most forms of ferments to spoil the paper, and if the latter can be thoroughly dried the action of the ferment is delayed and stopped, although it may revive again if the paper is dampened at some subsequent time before the ferment loses all its vitality. The manufacturer should, therefore, be careful both to dry the paper as quickly as is consistent with its quality and to finally heat it or dry it so thoroughly as to kill the ferment and to insure the indefinite preservation of the paper.

It is in the process of drying that the difficulties introduced by our variable climate come into play. The drying room and the processes that give satisfactory results in dry weather, or average moist weather, are very apt to fail when a long series of moist days supervenes or when an abundance of some special fungus is present. If the quality of the paper will allow it, it is

simplest and surest to dry the web in one continuous process as it passes from the vat to the end of a series of several hundred rolls; but in making finer qualities of calendered and highly-sized papers the separate sheets are hung up in the drying room, and it is in this latter case that the danger of souring is greatest. It will not do to rely on the drying due to the free action of the wind blowing through the loft, not only because of the dust that is thus brought in, but also because the wind is so often laden with moisture. Neither will it do to simply close the loft and exclude the wind, because by this process we should saturate the air of the drying room, and in a few hours all drying would cease and the conditions would be most favorable to the rapid action of the souring ferments. It is necessary to remove the moisture from the drying loft, but it is not necessary to remove the air, as such; it is also best to conduct the whole operation at the ordinary low temperatures; it is also desirable to exclude the outside air, both because of its dust and of its steady supply of fungi. The whole problem is well solved by the following simple arrangement: —

Around the sides of the drying loft are ranged a system of straight iron pipes through which there is maintained a slow circulation of cold water. As soon as the dew point of the air within the room has, by the evaporation from the moist paper, been raised to a temperature slightly higher than that of the cold water pipes these latter begin to condense upon themselves the invisible vapor of the surrounding air. As dry air is heavier than moist air, at the same temperature, therefore the dry air slowly sinks and a fresh supply of moist air comes in contact with the pipes. Thus a circulation of air is maintained throughout the drying loft, and the moisture (which condenses on the pipes as fast as the air brings it to them from the damp paper) drops from the pipes into gutters which convey it rapidly outside of the room. This process can be adjusted to suit the desires of the manufacturer by simply altering the quantity of the flowing cold water and the relative temperature of the water and the air. By this means an uniform percentage of dryness, usually about 50 or 60 per cent of relative humidity and a fairly uniform temperature, usually between 60 and 90 degrees, may be maintained, such as are best adapted to the perfect preparation of the paper.

About the year 1878 letters came to me complaining of difficulties in connection with the drying of codfish, both at the fisheries and at the repacking establishments, similar to those experienced in the drying of paper. As the meteorologist could not control the dryness of the free atmosphere, the best he could do to assist these industries was to direct their attention to these mechanical appliances by which they might offset the injury done by the weather. Of course the rapid process of evaporation into a vacuum, maintained by a steam engine and air pump, perfectly responds to all the conditions of successful manufacture, provided only the expense be not objectionable. From an economic point of view I think that the introduction of the vacuum drying pan, or the centrifugal dryer, would be a profitable improvement in the preparation of codfish and herring for market.

A very important field of study has developed during the past thirty years, viz., the examination of the quantity and character of the spores or germs floating in the air. A plate covered with a thin layer of syrup, or glycerine, or gelatinized broth is exposed for a minute to the wind and then placed under a glass case in a warm room. If any germs are caught that can live in this gelatinous layer then their development can be studied under the microscope at leisure. As dust is a component part of our atmosphere and affects its temperature, optical phenomena, and rainfall, we must include this study of atmospheric dust as a branch of Meteorology, the proper name of which would naturally be Koniology (*Κονις*, dust).

WASHINGTON, Oct. 10, 1892.

THE SLING PSYCHROMETER.

PROF. H. A. HAZEN.

THERE has just come to hand a long discussion of the aspiration psychrometer by Dr. Assmann of Berlin. See "Abhandlungen des Königlich Preussischen Meteorologischen Instituts, Berlin, 1892." This memoir is very complete and leaves nothing to be desired in its 153 quarto pages save, perhaps, a few comparisons between the aspiration psychrometer and Regnault's condensing hygrometer. The importance of settling

upon a rigid formula for the ventilated psychrometer will be recognized at once. A sling psychrometer is by all odds the most convenient instrument for use in a balloon and in innumerable other places. Moreover, when properly manipulated, it will give accuràte results very quickly. It is a little surprising that, in the recent meteorologic tables published by the international committee at great expense, there is no formula for the psychrometer.

A few comparisons have been made between the aspiration psychrometer and the condensing hygrometer; and the formula for the psychrometer computed by Dr. Sprung is to be found at page 235 in the above paper.

Dr. Assmann has attempted a few comparisons between the sling and aspiration psychrometers, and has found that the latter gives a somewhat lower temperature. I presume this is due, in most cases, to the use of the sling psychrometer in direct sunshine, or where reflected heat affected its readings. I have, personally, made most careful comparisons and have found no difference between the two. If the sling psychrometer is to be used out of doors during sunshine it should be in the shade of a tall tree or building. But there is a still stronger proof that the sling and aspiration psychrometers give identical results in that the formulae obtained for each independently are exactly the same.

The ordinary psychrometer formula is as follows: —

$$x = f - A\,(t - t')\,p.$$

In which x and f are vapor pressures at dew point and wet bulb temperatures, t and t′ are dry and wet bulb temperatures, p is air pressure and A a constant to be determined.

In this JOURNAL for February, 1885, I gave the results of a large number of comparisons between the sling psychrometer and Allnard's condensing hygrometer. The value of A in the formula I gave as .000678 at p. 397, and stated that from the observations this value was slightly too great. Subsequently this value was found not far from .00067, which was the result printed in the Austrian Journal, 1885, p. 92.

In 1885, Prof. Marvin, of the Signal Office, spent several months on Pike's Peak and at its base, in making comparisons between the sling psychrometer and Regnault's condensing

hygrometer. These observations with a large number of others at sea level were used by Prof. Ferrel in determining a formula which he has given in the annual report of the Chief Signal Officer for 1886, p. 249. I will give these three formulae reduced to a common measure and then a few comparisons by actual examples: —

$$\text{Sprung}: - x = f - .000662\ (t - t')\ p.$$

$$\text{Ferrel}: - x = f - .000660 \left\{ 1 + \frac{t - t'}{1571} \right\} (t - t')\ p.$$

$$\text{Hazen}: - x = f - .000670\ (t - t')\ p.$$

Let $t = 80$, $p = 30''$ and t' values below, then we have the following dew points: —

	t' 70	60	50
Sprung	65.2	44.8	−9.4
Ferrel	65.2	44.6	−13.3
Hazen	65.2	44.6	−12.3

The results in this table are very remarkable and show that these instruments, so very diverse in character, are almost perfectly in accord. For at least 80 per cent of observations there is no difference between the formulae. At extremes of dryness there were very few observations for determining the correct formula so that the slight discrepancies are not to be wondered at. I do not think much weight should be given to the fact that at extreme dryness the last result in this table comes between the other two.

The above was written several months ago, and was laid aside for other matter which seemed more interesting. There has just come to hand, in the October number of this Journal, a criticism of the sling psychrometer which ought not to pass unnoticed. At page 250, in the description of a balloon voyage near Berlin, Germany, I find: "The most important result of the ascent, however, was furnished by the comparison between the aspiration and sling psychrometers, which confirmed the opinion of Dr. Assmann regarding the untrustworthiness of the latter. As a fact, the air temperatures obtained by this instrument, with rather intense radiation, were always higher than those given by the aspiration thermometer, the difference averaging about 2° C (3.6° F.) and varying from .3° (.5°) to 3° (5.°4).

The relative humidities obtained with the aspiration psychrometer were generally from 0 to 9 per cent above those with the sling, the wet-bulb of the former being less depressed with respect to the dry bulb than was the wet bulb of the latter instrument. These results seem to demonstrate that the sling psychrometer, as used in a balloon, where it cannot be swung far away from the basket, and under intense insolation, gives values which are not only too high, but which do not follow the constantly changing temperature and humidity of the air."

This is a rather unfortunate statement of the actual behavior of a sling psychrometer. When properly understood, we here seem to have a positive proof of the untrustworthiness not of the sling, but of the aspiration psychrometer in a rapidly moving balloon. These observations were made about 800 miles farther north than Washington, D. C., hence the intensity of the insolation was appreciably less, even in free air, than here at the earth. Most careful experiments have been made which have shown that under the most intense insolation the sling psychrometer reads from .7° to 1.°0 F. higher in the sun than in the shade. There is no place in the world or in the free air where the sling can, by any possibility, read 5.4° F. higher than the true air temperature at the point of observation. The details of the above balloon comparison are not given; but I venture to state that these great differences were probably found with a rather rapidly descending balloon and were due almost wholly to the error (lagging) of the aspiration thermometer. Whatever may be the truth, however, it is absolutely certain that this difference was not due to intense insolation or to an actual difference of temperature in the air at the points of observation. In ordinary cases, I very much doubt if the sling thermometer reads more than .5 too high in bright sunshine, but it is never necessary to use this instrument in the sun. There is no difficulty at all in rigging a flag or a curtain so as to shut off the direct sun's heat. But this is not all. The muslin covering of the wet bulb is a vastly better absorber of the sun's heat than the glass of the dry with its backing of a perfectly polished surface; and it will be found invariably that the rise in temperature of the wet in bright sunshine is *greater* than that of the dry; not only is this true on theoretical grounds, but I have tested it by hundreds of observations in bright sun and in shade. Whatever doubt any one

may have as to the amount of the effect of insolation on the dry there can be absolutely no doubt that in *bright sunshine the relative humidity given by a psychrometer will always be greater than in the shade.* It is difficult to see how this error in the above extract arose; it is possible, of course, that there is a misprint and the intention was to show that the sling gave higher humidity. If any one doubts the points here made he can satisfy himself very quickly by a few simple experiments. Let him choose the shade of a tall tree, the shade of a wall will not do because there the actual temperature of the air on a still day will be less than in the sun. Now, carefully observe the temperature of the sling in the shade of the tree and at once go into bright sunshine and repeat the observation. Do this several times to eliminate accidental errors. I have done this so often that I know the statements made above cannot be gainsaid.

The great advantages of the sling are its extreme portability, ease of handling, and rapidity of action, not one of which advantages is possessed by any other apparatus. If one understands it, the muslin can be kept in perfect condition even with ice on the bulb, the thermometers are close at hand and can be read with great ease and certainty, the motion of the sling may be increased to twenty-five miles an hour, if need be, and observations made every three-fourths minute. To any one who has had experience with the sling; in the attempt to wet an aspiration muslin, to read the thermometers once in five minutes by a telescope, to wind the clock work which possibly gives a velocity of fresh air at the immediate bulb of five miles per hour; it is very easy to see why in a balloon descending 200 feet in a minute the above results should have been so strongly in favor of the sling psychrometer. I very much hope in the near future to make observations with a balloon rising or falling at the rate of 500 feet per minute; and I have no doubt the sling will prove itself the perfect instrument that it always has in the past, giving almost identical results in the same layer of air going up and coming back, something never obtained before with any other instrument.

OCT. 17, 1892.

THE ASPIRATION vs. THE SLING PSYCHROMETER.

A. LAWRENCE ROTCH.

I BEG to make the following reply to Prof. Hazen's criticism of the aspiration psychrometer: —

Respecting Dr. Assmann's memoir which Prof. Hazen reviews, I will only remark that the "somewhat lower temperatures" quoted by Prof. Hazen, as given by the aspiration when compared with the sling thermometer, are stated by Dr. Assmann to reach 1.5° C. (2.7° F.) with intense insolation and in general to be 0.8° C. (1.4° F.)

Dr. Assmann also states that the humidity determinations made with the sling only agree with those deduced from the aspiration psychrometer when, for the former Jelinek's tables, and for the latter, Sprung's formula, $=f'-\frac{1}{2}(t-t')\frac{b}{755}$, are used.

Regarding Prof. Hazen's criticism of my observations made in a free balloon with an aspiration and a sling psychrometer (see this JOURNAL, October, 1892), I have examined the original figures upon which my statements were based, and find that they justify my conclusions that the Assmann aspiration psychrometer is superior to the sling psychrometer for use in balloons. I know from my own experiments that readings in free air of the sling thermometer in the sun (even under the intense insolation of the Sahara) and in the shade usually differ less than 0.5° F., and it is possible that comparisons with the aspiration instrument under those conditions may not show larger differences. Under exceptional circumstances, however, such as on high glaciers and snow fields, the readings of the aspirated dry bulb have been in bright sunshine from 0.8° C. to 2.4° C. (1.4° to 4.3° F.) below a sling similarly exposed near the ice or snow.

As I have stated, the sling is not used to advantage in a balloon, since it cannot be swung far enough outside the basket to be removed from the heat radiated by the balloon and the envelope of air which is dragged with it. The aspiration, however, is exposed at a distance of six or eight feet from the basket, and is, therefore, outside this heated air, while, by its construction, it is protected from radiation. This psychrometer can also

be read more frequently, for if Prof. Hazen will refer to the description of the Assmann psychrometer in this JOURNAL, (Vol. VIII., page 215), he will see that two wet-bulb thermometers are used which are wet, and read alternately; and, therefore, since the aspiration is continuous, the thermometers at all times give the true temperatures, except during one or two minutes in every ten, when the instrument is pulled up to the basket for re-winding and re-wetting. With the sling, on the contrary, as ordinarily used, time must be allowed, after wetting the wet bulb, for the water to acquire the temperature of the air; and the disadvantage of intermittent slinging is not made up by increasing the speed of whirling, since for the aspiration psychrometer, at least, according to Dr. Assmann, no advantage is gained by accelerating the flow of air past the bulb above 2.3 meters per second (5 miles per hour), which is about the velocity of the air in his apparatus.

Coming now to the question of sensitiveness of the two instruments, and to my statement that the relative humidities obtained with the aspiration psychrometer, in the balloon voyage described, were generally above those found with the sling, because the wet bulb of the former was generally less depressed with respect to the dry bulb than the wet bulb of the sling, I give below all the simultaneous readings with the two instruments. The vertical motion of the balloon at the moment of observation was obtained from the self-recording barometer. It will be seen that, contrary to Prof. Hazen's supposition, the largest temperature difference (3.0° C.) occurred when the balloon was rising, which seems to show that this and the other differences are in part due to the sluggishness of the sling rather than that of the aspiration. It is seen that the relative humidities obtained with the aspiration psychrometer are slightly higher than the humidities obtained with the sling psychrometer, the three cases when the sling psychrometer gave decidedly higher values occurring with a balloon floating at nearly a uniform level, and, therefore, in a stratum of air which was not rapidly changing its hygrometric state. The mean difference between the aspiration and sling dry-bulbs is seen to be slightly greater than the difference between the corresponding wet bulbs.

PSYCHROMETER OBSERVATIONS DURING BALLOON ASCENT OF OCT. 24, 1891.

Time.	Alt. in Metres and Vert. Motion of Balloon.	Dry Bulb. Sling.	Dry Bulb. Aspiration.	Dry Bulb. Diff.	Wet Bulb. Sling.	Wet Bulb. Aspiration.	Wet Bulb. Diff.	Relative Humidity. Aspiration.	Relative Humidity. Sling.	Relative Humidity. Diff.
		Centigrade Degrees.			Centigrade Degrees.			Per Cent.		
10.41	254 rising	13.2	12.5	0.7	11.1	10.6	0.5	80	77	3
45	353 "	13.2	12.4	0.8	11.2	10.5	0.7	80	78	2
47	485 "	13.1	11.6	1.5	11.1	10.1	1.0	82	78	4
50	587 "	11.8	11.6	0.2	10.4	9.3	1.1	83	85	-2
58	783 "	10.8	10.8	0.0	10.1	9.2	0.9	82	81	1
11.06	872 "	12.5	10.3	2.2	9.8	8.0	1.8	74	71	3
10½	918 stationary	13.0	10.4	2.6	9.8	7.4	2.4	67	66	1
18	984 "	11.3	10.5	0.8	8.5	7.3	1.2	65	68	-3
25	965 rising	13.2	10.7	2.5	10.5	7.8	2.7	69	71	-2
30½	993 stationary	11.3	10.2	1.1	9.1	7.3	1.8	68	75	-7
36½	1,097 "	11.8	9.7	2.1	9.0	7.1	1.9	71	69	2
46	1,121 "	11.8	9.4	2.4	8.8	7.2	1.6	75	67	8
51	1,084 "	11.1	10.0	1.1	8.5	7.3	1.2	70	70	0
58	1,071 falling	11.5	9.9	1.6	8.7	7.5	1.2	73	68	5
12.05	1,074 "	11.8	9.5	2.3	8.5	6.9	1.6	72	63	19
11	1,008 "	12.2	10.0	2.2	8.8	7.2	1.6	69	63	6
20	898 "	12.1	10.4	1.7	9.0	7.2	1.8	66	66	0
25	957 stationary	12.3	10.3	2.0	9.0	7.1	1.9	67	76	-9
59	1,189 falling	10.0	8.5	1.5	7.5	6.2	1.3	73	70	3
1.10	1,107 rising	13.0	1c.0	3.0	9.5	7.2	2.3	69	64	5
20	1,234 "	10.5	8.6	1.9	7.5	6.2	1.3	72	66	6
Mean differences,		-	-	1.6	-	-	1.5		-	1.17

Blue Hill Observatory, December, 1892.

CURRENT NOTES.

The Recent Rainfall Experiments at San Antonio, Texas. — Prof. Alexander Macfarlane, of the University of Texas, contributes to the San Antonio *Daily Express*, of Dec. 4, an interesting article on the recent experiments in artificial rainfall made near that city under the direction of Mr. R. G. Dyrenforth, whose name is already familiar to our readers. These experiments were paid for by a number of Chicago capitalists who were interested in the scheme as a source of gain to themselves, if successful. The plan adopted was similar to that already used in the preceding experiments, viz., by producing great concussions to imitate the conditions of a battle, and so to cause the rain which the supporters of the rain-making theory say usually follows battles.

Prof. Macfarlane describes in detail the operations on one day. At 9 A. M. the temperature was 72°, the dew point 60°, and the wind southeast. A slight rain fell between 7 and 8.30, and throughout the forenoon the sky was completely overcast. Operations began at 4 P. M., when the sky was almost completely overcast with rain clouds, the sun shining through occasionally. The first shell was fired at 4 o'clock, and at 4.20 a slight rain fell. At 4.50 the explosions of rosellite were begun, and were continued at the rate of two explosions a minute for about an hour. At 5.28 a canvas oxy-hydrogen balloon was sent up and exploded, but no effect was produced. At 5.48 the sky was clearer than at the beginning of the operations, but the moon was surrounded by a halo. At 6.20 operations ceased, to allow the men time for supper. At 7.30 the sky was overcast, and the explosions were continued as before. At 10.15 a balloon was sent up and exploded, without noticeable effect. At 10.45 it began to mist, but ceased at 11.40, when the stars began to appear. At 1 A. M., when operations were to be suspended for the night, it was fair, with some stars visible; but the explosions were kept up. At 1.30 it began to rain, and ceased again at 2. The rain that fell was a mere sprinkle, the gauge indicating 0.01 in. Prof. Macfarlane concludes as follows: "The trial of Friday was a crucial test, and resulted not only in demonstrating what every person who has any sound knowledge of physics knows, that it is impossible to produce rain by making a great noise, but also that even the explosion of a twelve-foot balloon inside a black rain cloud does not bring down a shower."

Royal Meteorological Society. — THE first meeting of the session was held on Wednesday evening, Nov. 16, at the Institution of Civil Engineers, 25 Great George Street, Westminster, Mr. A. Brewin, Vice-President, in the chair.

Mr. E. T. Adams, Mr. A. L. Jones, M. R. C. S., Mr. J. E. Prince, and Mr. W. Tattersall, C. E., were elected Fellows of the Society.

An interesting paper by Mr. J. Lovel was read on the "Thunderstorm, Cloudburst, and Flood at Langtoft, East Yorkshire, July 3, 1892." The author gives an account of the thunderstorm as experienced at Driffield on the evening of this day; the full force of the storm was, however, felt in the wold valleys which lie to the north and northwest of Driffield, where great quantities of soil and gravel were removed from the hillsides and carried to the lower districts, doing a large amount of damage. Many houses in the lower parts of Driffield were flooded, and a bridge considerably injured. The storm was most severe in a basin of valleys close to the village of Langtoft, where three trenches, sixty-eight yards in length and of great width and depth, were scooped out of the solid rock by the force of the water from the cloudburst. From the appearance of the trenches, it is probable that there were three waterspouts moving abreast simultaneously. This particular locality seems to be favorable for the formation of cloudbursts, as there are records of great floods having previously occurred at Langtoft, notably on April 10, 1657, June, 1857, and June 9, 1888. The author gives, in an appendix, a number of observations made on similar occurrences, together with particulars and opinions as to the cause of such outbursts by several eminent authorities.

Mr. W. H. Dines also read a paper: "Remarks on the measurement of the maximum wind pressure, and description of a new instrument for indicating and recording the maximum." For some years the author has been conducting a large number of experiments with various forms of anemometer; and in the early part of the present year recommended the adoption of the tube anemometer for general use, as it appeared to possess numerous advantages. The head is simple in construction, and so strong that it is practically indestructible by the most violent hurricane. The recording apparatus can be placed at any reasonable distance from the head, and the connecting pipes may go round several sharp corners without harm. The power is conveyed from the head without loss by friction, and hence the instrument may be made sensitive to very low velocities without impairing its ability to resist the most severe gale. In the present paper the author describes an arrangement of this form of anemometer which he has devised for indicating very light winds as well as recording the maximum wind pressure.

*Cotton Fibres and Atmospheric Changes.** — The value of the reports of the United States Department of Agriculture, Weather Bureau, are becoming more apparent every year. Not only is this the case on the coast and in the agricultural districts, but in the manufacturing centres as well; and especially is this true in the cotton mills in New England where large invoices of cotton are consumed annually.

It is a well known fact that the temperature has quite an important bearing upon cotton fibres during the manipulation from the bale to the cloth room. This must be evident to the most casual observer when we consider the fact

* From the Bulletin of the New England Weather Service for September, 1892.

that cotton is grown in a warm climate surrounded by a mean temperature of 70°, and then transmitted to a climate that is subject to sudden atmospheric changes, many of them being of low temperature, and with an atmosphere divested of moisture.

Cotton fibres are very susceptible to any atmospheric change; that is to say they will take on or throw off dampness very readily; consequently, any material change of temperature and humidity will affect the successful working of the fibre. In order for cotton to work well in the first processes of manipulating, the dry bulb thermometer of the common psychrometer should stand at 78°, and the wet bulb at 66°, which would make the dew point 58°, and, the relative humidity 52 per cent. This would give 5.371 grains of water vapor per cubic foot of air. Cotton fibres, with this condition of atmosphere, will very readily assimilate and draw even. During very many of the atmospheric changes that are constantly taking place it is found quite impossible to hold the cotton fibres well in hand. This is more noticeable where dry atmosphere prevails with a dry W. N. W. wind blowing for twenty-four or thirty-six hours. It is frequently the case, where these changes take place, that the amount of water vapor in a cubic foot of air will drop as low as 4.290 grains. When these conditions occur the electrical currents of air seriously interfere with the workings of cotton fibres. Electricity causes the fibres to separate, and much more waste is made. Very many of the cotton mills are not supplied with moistening apparatus.

The above shows the great value of the weather forecasts and also of daily observations, as with them the proper adjustments can be made at the machines with, or slightly in advance of, the weather changes.

F. E. Saunders.

Lowell, Mass., Oct. 1, 1892.

CORRESPONDENCE.

ARE OUR WINTERS BECOMING MILDER?

Editor of the American Meteorological Journal: —

As there seems to be a general opinion prevailing that the climate of our country is changing, especially with reference to the winters, which, it is claimed, are growing milder, I should think a discussion bearing on this subject by some of your contributors would be timely and pertinent.

The above opinion does not seem to be confined to the people of any particular climate or latitude, as I have encountered it at each section in which I have been located ; and with a view of finding if there was any real grounds for this assumption, I have at each station examined the records of the Weather Bureau relative to this matter.

I have found, in each instance, that the records show that no perceptible change has taken place in the climate ; and therefore I have always claimed that the idea is an erroneous one, and that the change in climate is only apparent and not real.

It is true that I have, in some instances, found a succession of mild winters, but in other cases a succession of extremely severe winters would be found, which would bring about an evening up, so that the average would really be the same.

It has also been my experience that whenever there occurs a period of extreme heat, a long wet or dry spell or a period of exceedingly fine weather, people will always claim that no such weather ever occurred before, when if the records are searched it can be found that just such weather, or even worse, has occurred many times, and in some instances the records will show that only the average weather for that particular locality prevailed during the period in question.

I should be pleased to see through the columns of your valuable JOURNAL an article on this subject from some one who has perhaps given the matter more thorough study and thought than I have done, as I am, and many of your readers doubtless are, interested in the subject.

S. L. DOSHER,
Observer, Weather Bureau.

MANISTEE, MICH., Dec. 20, 1892.

EFFECT OF TOPOGRAPHY ON THE PRESSURE OF THE WIND.

Editor of the American Meteorological Journal:—

The note in the September number of the JOURNAL on the "Effect of high winds on the barometer," by H. Helm Clayton, of the Blue Hill Observatory, induces me to present the following memorandum which may be of assistance, both in the study of the subject at high level observatories, and in making barometric measurements for elevation at hitherto unoccupied mountain points.

The island of Oahu presents a mountain range or ridge, parallel to its northeastern coast, and at right angles to the prevailing trade wind. This range averages in height about twenty-five hundred feet, and is precipitous on the weather side for an extent of nearly thirty miles. From the town of Honolulu, on the leeward coast, the Nuuanu Valley extends inland, not entirely cutting the range in twain, but ending in a pass known as the "Pali," twelve hundred feet above sea level, the weather side of which is a precipice six hundred feet high, forming a part of the wind-obstructing wall mentioned above. The bottom of the valley is on a moderate grade all the way up six miles to the Pali, which is a point much frequented by visitors.

Some years since I took occasion to make a measurement of height of the Pali by mercurial barometer observations, compared with simultaneous readings at Honolulu. The trigonometrical determination of the same elevation was made about the same time, and subsequently verified by actua levelling. At the summit the barometric height exceeded the true height by about twenty-five feet. But at a point nearly two miles down the valley, of eight hundred and fifty feet elevation, the results by barometer and spirit level practically agreed. This difference was attributed at the time to some imperfection either in the observations or in the formula, and no more was thought of the matter. Lately, however, I took a first class aneroid barometer on a trip up the valley, and with precisely the same result, viz., correct everywhere excepting near the brink of the weather-side precipice. There had not time enough elapsed, either, during the ascent for the usual progressive change which is characteristic of all aneroids at greater altitudes.

The trade wind draws through the pass with great force, and sometimes at the turning point with terrific violence. It strikes the face of the cliff below and shoots upward, as is seen by the mists that are driven up the heights on either side. Just a very short distance away, and where one would naturally observe for height, is a comparatively calm area, and, doubtless, a partial vacuum caused by the above-mentioned upward movement. Lower down the valley, *i. e.*, toward the town, the wind resumes its regular progressive force. This accounts, no doubt, for the barometer standing lower than it should at the summit station, and thus indicating too great an altitude, while at the lower station the reading was normal for the true height.

Moreover, a good observer, Mr. Henry Cobb-Adams, has lately taken observations for the Royal Meteorological Society (London) at a station at

Kaneohe on the weather side of the island. The situation is a narrow strip of plain along the foot of the mountain. He has kindly furnished this government with copies of his record, from time to time, and although our readings were alike while he was at his previous station in Honolulu, yet on the other side of the island his averages are uniformly higher, and his single observations — synchronous — vary from equality to 0.04 inch higher than the Honolulu record for the same time. This difference is apparently the greatest when the trades are blowing freshest, and disappears with a southerly wind, even sometimes turning the other way in that case. We have not yet tested the matter in a storm wind. It is evident that the pressure of flowing air against the face of the mountain causes a high barometer area all along the plain in front of it. So, then, were the Kaneohe observations used for comparison with a simultaneous set at the Pali, a height erroneous by even sixty feet might be deduced. My observations mentioned above were during moderate trades.

Notes that I have seen in scientific journals on the observations at Ben Nevis doubtless point to a similar state of things, and a similar explanation. It cannot be called directly an "effect of high winds on the barometer" so much as an effect of the configuration of the ground on country on the pressure of the wind, and an effect which should be thought of in determining previously unknown mountain heights by the atmospheric pressure.

With regard to the suggestion by Mr. Clayton that the "suction" of a hurricane across a ship's deck would produce a partial vacuum in the hold or cabin, it may possibly be a question whether so limited a space would be sufficient for that effect, viz., to produce a fall of one or two tenths of an inch. A little judicious experimenting may settle the doubt.

CURTIS J. LYONS.

HONOLULU, November, 1892.

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"Meteorologische Zeitschrift," Vienna. October, 1892. This number contains an interesting article entitled "Klima des Puy de Dome in Centralfrankreich," by Dr. Woeikof. The author reviews the records made at the mountain station of the Puy de Dome, and at the base station, Clermont, which have been published in the "Annales du Bureau Central Meteorologique." Both of these stations are admirably situated, the one for mountain, the other for valley, observations, and the study of the records gives some interesting results. In the valley, complete saturation of the air is seldom noted, and when noted is in the months from September to March, while on the summit the air is saturated more than one third of the time at 6 a. m. and at 9 p. m. During anticyclones, when the air is descending, the relative humidity at the summit is very low, while at the base the temperature may be much lower and the air very moist. The mean temperature of two months, December, 1879, and January, 1882, was higher at the summit than at the base; and conditions of warm and dry air on the summit and moist cold air at the base often last for weeks. The vertical decrease of temperature aloft is more rapid at the Puy de Dome than in the Alps, but slower than at Ben Nevis and the Brocken. As regards precipitation, the

amount measured on the summit is two and one half times that at the base. The Puy de Dome is not high enough to reach into the upper wind system, and shows similar wind directions as the valley. On the other hand, the wind velocity has the characteristic noted on higher isolated mountains, viz., a minimum velocity during the warm hours of the day. The maximum velocity for the year at the summit is in December, the minimum in August.

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